to the British list; and of these it appears that no less than thirty-three have been first described by Mr. Gosse, twelve of them in the work now under consideration. Moreover, for the discovery of twelve of the new species we are indebted to our author; so that he may put in a strong claim to be considered the historian of the British Sea-Anemones. Amongst the additions, it is interesting to see that no less than ten species of Coralligenous Polypes occur in our seas, Johnston only describing three, if we omit the *Pocillopora interstincta*, which is inserted by Mr. Gosse with a note of interrogation.

As we have already described the mode in which Mr. Gosse has treated his subject, it will be unnecessary to enter upon its consideration here, further than by stating that he has executed his plan most judiciously throughout; his descriptions are clear and characteristic; and the habits of the animals are treated of in that agreeable manner which must be familiar to all readers of Mr. Gosse's books. The system adopted by Mr. Gosse in conferring English names upon the Sea-Anemones is also worthy of notice, as he has, by a bold manufacture of diminutive names, most happily succeeded in avoiding those sesquipedalian combinations which usually render the so-called English names of animals more uncouth and unpronounceable than their scientific denominations.

The last Part contains an Index and an Introduction, the latter giving a description of the anatomy and physiology of the Helianthoid Polypes, which will be found of great service to the student, especially as so many of the anatomical terms now adopted for these and many other groups of the lower animals are not to be found in any of our zoological text-books. We have already spoken of the great beauty of the illustrations, and may therefore now take leave of Mr. Gosse's book, in the hope that many of our readers will avail themselves of such an excellent guide in the investigation of the interesting order of animals to which it is devoted.

MISCELLANEOUS.

Darwin *on the Origin of Species.*

By Prof. Asa Gray, Cambridge, United States *.

[In our Number for September last we placed before our readers an extract from the forthcoming volume of Prof. Agassiz's 'Contributions to the Natural History of the United States,' relating to the interesting question as to the origin of species, newly raised by Mr. Darwin's well-known book. We now give a notice on the opposite side of the question to that taken by Prof. Agassiz, from the pen of another able naturalist of the United States, for the communication of which we are indebted to Mr. Darwin.—Eds.]

"I can entertain no doubt, after the most deliberate study and dispassionate judgment of which I am capable, that the view which most naturalists entertain, and which I formerly entertained—namely that each species has been independently created—is erroneous. I

* From the 'Atlantic Monthly,' August 1860.

am fully convinced that species are not immutable, but that those belonging to what are called the same genera are lineal descendants of some other and generally extinct species, in the same manner as the acknowledged varieties of any one species are the descendants of that species. Furthermore, I am convinced that Natural Selection has been the main, but not exclusive, means of modification."

This is the kernel of the new theory—the Darwinian creed, as recited at the close of the introduction to the remarkable book under consideration. The questions "What will he do with it?" and "How far will he carry it?" the author answers at the close of the volume: "I cannot doubt that the theory of descent with modification embraces all the members of the same class." Furthermore, "I believe that all animals have descended from at most only four or five progenitors, and plants from an equal or lesser number." Seeing that analogy as strongly suggests a further step in the same direction, while he protests that "analogy may be a deceitful guide," yet he follows its inexorable leading to the inference that "probably all the organic beings which have ever lived on this earth have descended from some one primordial form, into which life was first breathed *."

In the first extract we have the thin end of the wedge driven a little way; in the last, the wedge is driven home.

We have already sketched some of the reasons suggestive of such a theory of derivation of species—reasons which give it plausibility, and even no small probability, as applied to our actual world and to changes occurring since the last tertiary period. We are well pleased at this moment to find that the conclusions we were arriving at in this respect are sustained by the very high authority and impartial judgment of Pictet, the Swiss palæontologist. In his review of Darwin's book †—much the fairest and most admirable opposing one that has yet appeared—he freely accepts that *ensemble* of natural operations which Darwin impersonates under the now familiar name of Natural Selection, allows that the exposition throughout the first chapters seems "*à la fois prudent et fort*," and is disposed to accept the whole argument in its foundations,—that is, so far as it relates to what is now going on, or has taken place inthe present geological period, which period he carries back through the diluvial epoch to the borders of the tertiary‡. Pictet accordingly admits that the

* Page 484, Engl. ed. In the new American edition (*vide* Supplement, pp. 431, 432) the principal analogies which suggest the extreme view are referred to, and the remark is appended—"But this inference is chiefly grounded on analogy, and it is immaterial whether or not it be accepted. The case is different with the members of each great class, as the Vertebrata or Articulata; for here we have in the laws of homology, embryology, &c., some distinct evidence that all have descended from a single primordial parent."

† In Bibliothèque Universelle de Genève, Mars 1860.

‡ This we learn from his very interesting article 'De la Question de l'Homme Fossile,' in the same (March) number of the Bibliothèque Universelle.

theory will very well account for the origination by divergence of nearly related species, whether within the present period or in remoter geological times,—a very natural view for him to take, since he appears to have reached and published, several years ago, the pregnant conclusion that there most probably was some material connexion between the closely related species of two successive faunas, and that the numerous close species, whose limits are so difficult to determine, were not all created distinct and independent. But while accepting, or ready to accept, the basis of Darwin's theory and all its legitimate direct inferences, he rejects the ultimate conclusions, brings some weighty arguments to bear against them, and is evidently convinced that he can draw a clear line between the sound inferences which he favours, and the unsound or unwarranted theoretical deductions which he rejects. We hope he can.

This raises the question, Why does Darwin press his theory to these extreme conclusions? Why do all hypotheses of derivation converge so inevitably to one ultimate point? Having already considered some of the reasons which suggest or support the theory at its outset,—which may carry it as far as such sound and experienced naturalists as Pictet allow that it may be true, perhaps as far as Darwin himself unfolds it in the introductory proposition cited at the beginning of this article,—we may now inquire after the motives which impel the theorist so much further. Here proofs, in the proper sense of the word, are not to be had. We are beyond the region of demonstration, and have only probabilities to consider. What are these probabilities? What work will this hypothesis do to establish a claim to be adopted in its completeness? Why should a theory which may plausibly enough account for the *diversification* of the species of each special type or genus be expanded into a general system for the *origination* or successive diversification of all species, and all special types or forms, from four or five remote primordial forms, or perhaps from one? We accept the theory of gravitation because it explains all the facts we know, and bears all the tests that we can put it to. We incline to accept the nebular hypothesis for similar reasons, not because it is proved—thus far it is wholly incapable of proof—but because it is a natural theoretical deduction from accepted physical laws, is thoroughly congruous with the facts, and because its assumption serves to connect and harmonize these into one probable and consistent whole. Can the derivative hypothesis be maintained and carried out into a system on similar grounds? If so, however unproved, it would appear to be a tenable hypothesis, which is all that its author ought now to claim. Such hypotheses as from the conditions of the case can neither be proved nor disproved by direct evidence or experiment are to be tested only indirectly, and therefore imperfectly, by trying their power to harmonize the known facts, and to account for what is otherwise unaccountable. So the question comes to this—What will an hypothesis of the derivation of species explain which the opposing view leaves unexplained? Questions these which ought to be entertained before we take up the arguments which have been advanced against this theory. We

can only glance at some of the considerations which Darwin adduces, or will be sure to adduce in the future and fuller exposition which is promised. To display them in such wise as to indoctrinate the unscientific reader would require a volume. Merely to refer to them in the most general terms would suffice for those familiar with scientific matters, but would scarcely enlighten those who are not. Wherefore let these trust the impartial Pictet, who freely admits that, "in the absence of sufficient direct proofs to justify the possibility of his hypothesis, Mr. Darwin relies upon indirect proofs, the bearing of which is real and incontestable," who concedes that "his theory accords very well with the great facts of comparative anatomy and zoology—comes in admirably to explain unity of composition of organisms, also to explain rudimentary and representative organs, and the natural series of genera and species—equally corresponds with many palæontological data—agrees well with the specific resemblances which exist between two successive faunas, with the parallelism which is sometimes observed between the series of palæontological succession and of embryonal development," &c.; and finally, although he does not accept the theory in these results, he allows that "it appears to offer the best means of explaining the manner in which organized beings were produced in epochs anterior to our own."

What more than this could be said for such a hypothesis? Here, probably, is its charm, and its strong hold upon the speculative mind. Unproven though it be, and cumbered *primâ facie* with cumulative improbabilities as it proceeds, yet it singularly accords with great classes of facts otherwise insulated and enigmatic, and explains many things which are thus far utterly inexplicable upon any other scientific assumption.

Darwin's hypothesis is the natural complement to Lyell's uniformitarian theory in physical geology. It is for the organic world what that popular view is for the inorganic; and the acceptors of the latter stand in a position from which to regard the former in the most favourable light. Wherefore the rumour that the cautious Lyell himself has adopted the Darwinian hypothesis need not surprise us. The two views are made for each other, and like the two counterpart pictures for the stereoscope, when brought together, combine into one apparently solid whole.

If we allow, with Pictet, that Darwin's theory will very well serve for all that concerns the present epoch of the world's history—an epoch which this renowned palæontologist regards as including the diluvial or quaternary period—then Darwin's first and foremost need in his onward course is a practicable road from this into and through the tertiary period, the intervening region between the comparatively near and the far remote past. Here Lyell's doctrine paves the way, by showing that in the physical geology there is no general or absolute break between the two, probably no greater between the latest tertiary and the quaternary period than between the latter and the present time. So far, the Lyellian view is, we suppose, generally concurred in. Now, as to the organic world, it is largely admitted

that numerous Tertiary species have continued down into the quaternary, and many of them to the present time. A goodly percentage of the earlier and nearly half of the later Tertiary Mollusca, according to Deshayes, Lyell, and, if we mistake not, Broun, still live. This identification, however, is now questioned by a naturalist of the very highest authority. But, in its bearings on the new theory, the point here turns not upon absolute identity so much as upon close resemblance. For those who, with Agassiz, doubt the specific identity in any of these cases, and those who say, with Pictet, that "the later Tertiary deposits contain in general the *débris* of species *very nearly related* to those which still exist, belonging to the same genera, but specifically different," may also agree with Pictet that the nearly related species of successive faunas must or may have had "a material connexion." Now the only material connexion that we have an idea of in such a case is a genealogical one. And the supposition of a genealogical connexion is surely not unnatural in such cases—is demonstrably the natural one as respects all those Tertiary species which experienced naturalists have pronounced to be identical with existing ones, but which others now deem distinct; for to identify the two is the same thing as to conclude the one to be ancestors of the other. No doubt there are differences between the Tertiary and the present individuals—differences equally noted by both classes of naturalists, but differently estimated. By the one these are deemed quite compatible, by the other incompatible with community of origin. But who can tell us what amount of difference is compatible with community of origin? This is the very question at issue, and one to be settled by observation alone. Who would have thought that the peach and the nectarine came from one stock? But this being proved, is it now very improbable that both were derived from the almond, or from some common amygdaline progenitor? Who would have thought that the cabbage, cauliflower, broccoli, kale, and kohlrabi are derivatives of one species, and rape or colza, turnip, and probably rutabaga, of another species? And who that is convinced of this can long undoubtingly hold the original distinctness of turnips from cabbages as an article of faith? On scientific grounds, may not a primordial cabbage or rape be assumed as the ancestor of all the cabbage races, on much the same ground that we assume a common ancestry for the diversified human races? If all our breeds of cattle came from one stock, why not this stock from the Aurochs, which has had all the time between the diluvial and the historic periods in which to set off a variation perhaps no greater than the difference between some sorts of cattle?

That considerable differences are often discernible between Tertiary individuals and their supposed descendants of the present day affords no argument against Darwin's theory, as has been rashly thought, but is decidedly in its favour. If the identification were so perfect that no more differences were observable between the Tertiary and the recent shells than between various individuals of either, then Darwin's opponents, who argue the immutability of species from the

ibises and cats preserved by the ancient Egyptians being just like those of the present day, could triumphantly add a few hundred thousand years more to the length of the experiment and to the force of their argument. As the facts stand, it appears that, while some Tertiary forms are essentially undistinguishable from existing ones, others are the same with a difference which is judged not to be specific or aboriginal, and yet others show somewhat greater differences, such as are scientifically expressed by calling them marked varieties, or else doubtful species; while others, differing a little more, are confidently termed distinct, but nearly-related species. Now, is not all this a question of degree, of mere gradation of difference? Is it at all likely that these several gradations came to be established in two totally different ways—some of them (though naturalists can't agree which) through natural variation, or other secondary cause, and some by original creation, without secondary cause? We have seen that the judicious Pictet answers such questions as Darwin would have him do, in affirming that, in all probability, the nearly-related species of two successive faunas were materially connected, and that contemporaneous species, similarly resembling each other, were not all created so, but have become so. This is equivalent to saying that species (using the term as all naturalists do and must continue to employ the word) have only a relative, not an absolute fixity; that differences fully equivalent to what are held to be specific may arise in the course of time, so that one species may at length be naturally replaced by another species a good deal like it, or may be diversified through variation or otherwise into two, three, or more species, or forms as different as species. This concedes all that Darwin has a right to ask, all that he can directly infer from evidence. We must add that it affords a *locus standi*, more or less tenable, for inferring more.

Here another geological consideration comes in to help on this inference. The species of the later Tertiary period for the most part not only resembled those of our days (many of them so closely as to suggest an absolute continuity), but also occupied in general the same regions that their relatives occupy now. The same may be said, though less specially, of the earlier Tertiary and of the later Secondary; but there is less and less localization of forms as we recede, yet some localization even in palæozoic times. While in the secondary period one is struck with the similarity of forms and the identity of many of the species which flourished apparently at the same time in all or in the most widely separated parts of the world, in the Tertiary epoch, on the contrary, along with the increasing specialization of climates and their approximation to the present state, we find abundant evidence of increasing localization of orders, genera, and species; and this localization strikingly accords with the present geographical distribution of the same groups of species. Where the imputed forefathers lived, their relatives and supposed descendants now flourish. All the actual classes of the animal and vegetable kingdoms were represented in the Tertiary faunas and floras, and in nearly the same proportions and the same diversities as at present. The faunas of what are now Europe, Asia, America, and Australia differed from

each other much as they now differ: in fact (according to Adolphe Brongniart, whose statements we here condense*), the inhabitants of these different regions appear for the most part to have acquired, before the close of the Tertiary period, the characters which essentially distinguish their existing faunas. The eastern continent had then, as now, its great Pachyderms, Elephants, Rhinoceros, and Hippopotamus; South America its Armadillos, Sloths, and Ant-eaters; Australia a crowd of Marsupials; and the very strange birds of New Zealand had predecessors of similar strangeness. Everywhere the same geographical distribution as now, with a difference in the particular area, as respects the northern portion of the continents, answering to a warmer climate then than ours, such as allowed species of Hippopotamus, Rhinoceros, and Elephant to range even to the regions now inhabited by the Rein-deer and the Musk-ox, and with the serious disturbing intervention of the glacial period within a comparatively recent time. Let it be noted also, that those Tertiary species which have continued with little change down to our days are the marine animals of the lower grades, especially Mollusca. Their low organization, moderate sensibility, and the simple conditions of an existence in a medium like the ocean, not subject to great variation, and incapable of sudden change, may well account for their continuance; while, on the other hand, the more intense, however gradual, climatic vicissitudes on land, which have driven all tropical and subtropical forms out of the higher latitudes and assigned to them their actual limits, would be almost sure to extinguish such huge and unwieldy animals as Mastodons, Mammoths, and the like, whose power of enduring altered circumstances must have been small.

This general replacement of the Tertiary species of a country by others so much like them is a noteworthy fact. The hypothesis of the independent creation of all species, irrespective of their antecedents, leaves this fact just as mysterious as is creation itself; that of derivation undertakes to account for it. Whether it satisfactorily does so or not, it must be allowed that the facts well accord with that assumption.

The same may be said of another conclusion, namely, that the geological succession of animals and plants appears to correspond in a general way with their relative standing or rank in a natural system of classification. It seems clear that though no one of the *grand types* of the animal kingdom can be traced back further than the rest, yet the lower *classes* long preceded the higher; that there has been on the whole a steady progression within each class and order; and that the highest plants and animals have appeared only in relatively modern times. It is only, however, in a broad sense that this generalization is now thought to hold good. It encounters many apparent exceptions, and sundry real ones. So far as the rule holds, all is as it should be upon a hypothesis of derivation.

The rule has its exceptions; but, curiously enough, the most striking class of exceptions, if such they be, seems to us even more

* In Comptes Rendus de l'Acad. des Sciences, Févr. 2, 1857.

favourable to the doctrine of derivation than is the general rule of a pure and simple ascending gradation. We refer to what Agassiz calls prophetic and synthetic types; for which the former name may suffice, as the difference between the two is evanescent.

"It has been noticed," writes our great zoologist, "that certain types, which are frequently prominent among the representatives of past ages, combine in their structure peculiarities which at later periods are only observed separately in different, distinct types. Sauroid fishes before reptiles, Pterodactyles before birds, Ichthyosauri before dolphins, &c. There are entire families of nearly every class of animals, which in the state of their perfect development exemplify such prophetic relations.... The Sauroid fishes of the past geological ages are an example of this kind. These fishes, which preceded the appearance of reptiles, present a combination of ichthyic and reptilian characters not to be found in the true members of this class, which form its bulk at present. The Pterodactyles, which preceded the class of Birds, and the Ichthyosauri, which preceded the Cetacea, are other examples of such prophetic types*."

Now these reptile-like fishes, of which Gar-pikes are the living representatives, though of earlier appearance, are admittedly of higher rank than common fishes. They dominated until reptiles appeared, when they mostly gave place to (or, as the derivationists will insist, were resolved by divergent variation and natural selection into) common fishes, destitute of reptilian characters, and saurian reptiles,—the intermediate grades, which, according to a familiar piscine saying, are "neither fish, flesh, nor good red-herring," being eliminated and extinguished by natural consequence of the struggle for existence which Darwin so aptly pourtrays. And so, perhaps, of the other prophetic types. Here type and antitype correspond. If these are true prophecies, we need not wonder that some who read them in Agassiz's book will read their fulfilment in Darwin's.

Note also, in this connexion, that, along with a wonderful persistence of type, with change of species, genera, orders, &c., from formation to formation, no species and no higher group which has once unequivocally died out ever afterwards reappears. Why is this, but that the link of generation has been sundered? Why, on the hypothesis of independent originations, were not failing species recreated, either identically or with a difference, in regions eminently adapted to their well-being? To take a striking case. That no part of the world now offers more suitable conditions for wild horses and cattle than the Pampas and other plains of South America, is shown by the facility with which they have there run wild and enormously multiplied, since introduced from the Old World not long ago. There was no wild American stock. Yet in the times of the Mastodon and Megatherium, at the dawn of the present period, wild horses and cattle—the former certainly very much like the existing Horse—roamed over those plains in abundance. On the principle of ori-

* Agassiz, 'Contributions:' Essay on Classification, p. 117, where, we may be permitted to note, the word "Crustacea" is by a typographical error printed in place of *Cetacea*.

ginal and direct created adaptation of species to climate and other conditions, why were these types not reproduced, when, after the colder intervening era, those regions became again eminently adapted to such animals? Why, but because, by their complete extinction in South America, the line of descent was here utterly broken? Upon the ordinary hypothesis, there is no scientific explanation possible of this series of facts, and of many others like them. Upon the new hypothesis, "the succession of the same types of structure within the same areas during the later geological periods ceases to be mysterious, and is simply explained by inheritance." Their cessation is failure of issue.

Along with these considerations, the fact should be remembered, that, as a general thing, related species of the present age are geographically associated. The larger part of the plants, and still more of the animals, of each separate country are peculiar to it; and, as most species now flourish over the graves of their by-gone relatives of former ages, so they now dwell among or accessibly near their kindred species.

Here also comes in that general "parallelism between the order of succession of animals and plants in geological times, and the gradation among their living representatives" from low to highly organized, from simple and general to complex and specialized forms; also "the parallelism between the order of succession of animals in geological times, and the changes their living representatives undergo during their embryological growth,"—as if the world were one prolonged gestation. Modern science has much insisted on this parallelism, and to a certain extent is considered to have made it out. All these things, which conspire to prove that the ancient and the recent forms of life "are somehow intimately connected together in one grand system," equally conspire to suggest that the connexion is one similar or analogous to generation. Surely no naturalist can be blamed for entering somewhat confidently upon a field of speculative inquiry which here opens so invitingly; nor need former premature endeavours and failures utterly dishearten him.

All these things, it may naturally be said, go to explain the order, not the mode, of the incoming of species. But they all do tend to bring out the generalization expressed by Mr. Wallace in the formula that "every species has come into existence coincident both in time and space with pre-existing closely allied species." Not, however, that this is proved, even of existing species, as a matter of general fact: it is obviously impossible to prove anything of the kind. But we must concede that the known facts strongly suggest such an inference. And since species are only congeries of individuals, and every individual came into existence in consequence of pre-existing individuals of the same sort, so leading up to the individuals with which the species began, and since the only material sequence we know of among plants and animals is that from parent to progeny, the presumption becomes exceedingly strong that the connexion of the incoming with the pre-existing species is a genealogical one.

Here, however, all depends upon the probability that Mr. Wallace's

inference is really true. Certainly it is not yet generally accepted; but a strong current is setting towards its acceptance.

So long as universal cataclysms were in vogue, and all life upon the earth was thought to have been suddenly destroyed and renewed many times in succession, such a view could not be thought of. So the equivalent view maintained by Agassiz, and formerly, we believe, by D'Orbigny, that, irrespective of general and sudden catastrophes, or any known adequate physical cause, there has been a total depopulation at the close of each geological period or formation, say forty or fifty times, or more, followed by as many independent great acts of creation, at which alone have species been originated, and at each of which a vegetable and an animal kingdom were produced entire and complete, full-fledged, as flourishing, as wide-spread and populous, as varied and mutually adapted from the beginning as ever afterwards,—such a view of course supersedes all material connexion between successive species, and removes even the association and geographical range of species entirely out of the domain of physical causes and of natural science. This is the extreme opposite of Wallace's and Darwin's view, and is quite as hypothetical. The nearly universal opinion, if we rightly gather it, manifestly is, that the replacement of the species of successive formations was not complete and simultaneous, but partial and successive, and that along the course of each epoch some species probably were introduced, and some, doubtless became extinct. If all since the Tertiary belongs to our present epoch, this is certainly true of it; if to two or more epochs, then the hypothesis of a total change is not true of them.

Geology makes huge demands upon time; and we regret to find that it has exhausted ours,—that what we meant for the briefest and most general sketch of some geological considerations in favour of Darwin's hypothesis has so extended as to leave no room for considering "the great facts of comparative anatomy and zoology" with which Darwin's theory "very well accords," nor for indicating how "it admirably serves for explaining the unity of composition of all organisms, the existence of representative and rudimentary organs, and the natural series which genera and species compose." Suffice it to say that these are the real strongholds of the new system on its theoretical side; that it goes far towards explaining both the physiological and the structural gradations and relations between the two kingdoms, and the arrangement of all their forms in groups subordinate to groups, all within a few great types; that it reads the riddle of abortive organs and of morphological conformity, of which no other theory has ever offered a scientific explanation, and supplies a ground for harmonizing the two fundamental ideas which naturalists and philosophers conceive to have ruled the organic world, though they could not reconcile them,—namely, Adaptation to Purpose and the Conditions of Existence, and Unity of Type. To reconcile these two undeniable principles is a capital problem in the philosophy of natural history; and the hypothesis which consistently does so thereby secures a great advantage.

We all know that the arm and hand of a monkey, the fore leg and

foot of a dog and of a horse, the wing of a bat, and the fin of a porpoise are fundamentally identical; that the long neck of the giraffe has the same and no more bones than the short one of the elephant; that the eggs of Surinam frogs hatch into tadpoles with as good tails for swimming as any of their kindred, although as tadpoles they never enter the water; that the Guinea-pig is furnished with incisor teeth which it never uses, as it sheds them before birth; that embryos of Mammals and Birds have branchial slits and arteries running in loops, in imitation or reminiscence of the arrangement which is permanent in Fishes; and that thousands of animals and plants have rudimentary organs which, at least in numerous cases, are wholly useless to their possessors, &c. Upon a derivative theory this morphological conformity is explained by community of descent; and it has not been explained in any other way.

Naturalists are constantly speaking of "related species," of the "affinity" of a genus or other group, and of "family resemblance," —vaguely conscious that these terms of kinship are something more than mere metaphors, but unaware of the grounds of their aptness. Mr. Darwin assures them that they have been talking derivative doctrine all their lives without knowing it.

If it is difficult, and in some cases practically impossible, to fix the limits of species, it is still more so to fix those of genera; and those of tribes and families are still less susceptible of exact natural circumscription. Intermediate forms occur, connecting one group with another in a manner sadly perplexing to systematists, except to those who have ceased to expect absolute limitations in nature. All this blending could hardly fail to suggest a former material connexion among allied forms, such as that which a hypothesis of derivation demands.

Here it would not be amiss to consider the general principle of gradation throughout organic nature,—a principle which answers in a general way to the law of continuity in the inorganic world, or rather is so analogous to it that both may be fairly expressed by the Leibnitzian axion, *Natura non agit saltatim*. As an axiom or philosophical principle, used to test model laws or hypotheses, this in strictness belongs only to physics. In the investigation of Nature at large, at least in the organic world, nobody would undertake to apply this principle as a test of the validity of any theory or supposed law. But naturalists of enlarged views will not fail to infer the principle from the phænomena they investigate,—to perceive that the rule holds, under due qualifications and altered forms, throughout the realm of Nature, although we do not suppose that Nature in the organic world makes no distinct steps, but only short and serial steps—not infinitely fine gradations, but no long leaps, or few of them.

To glance at a few illustrations out of many that present themselves. It would be thought that the distinction between the two organic kingdoms was broad and absolute. Plants and animals belong to two very different categories, fulfil opposite offices, and, as to

the mass of them, are so unlike that the difficulty of the ordinary observer would be to find points of comparison. Without entering into details, which would fill an article, we may safely say that the difficulty with the naturalist is all the other way—that all these broad differences vanish one by one as we approach the lower confines of the two kingdoms, and that no *absolute* distinction whatever is now known between them. It is quite possible that the same organism may be both vegetable and animal, or may be first the one and then the other. If some organisms may be said to be at first vegetables and then animals, others, like the spores and other reproductive bodies of many of the lower Algæ, may equally claim to have first a characteristically animal and then an unequivocally vegetable existence. Nor is the gradation purely restricted to these simple organisms. It appears in general functions, as in that of reproduction, which is reducible to the same formula in both kingdoms, while it exhibits close approximations in the lower forms; also in a common or similar ground of sensibility in the lowest forms of both, a common faculty of effecting movements tending to a determinate end, traces of which pervade the vegetable kingdom; while, on the other hand, this indefinable principle, this vegetable *animula vagula, blandula,* graduates into the higher sensitiveness of the lower class of animals. Nor need we hesitate to recognize the fine gradations from simple sensitiveness and volition to the higher instinctive and other physical manifestations of the higher brute animals. The gradation is undoubted, however we may explain it. Again, propagation is of one mode in the higher animals, of two in all plants; but vegetative propagation, by budding or offshoots, extends through the lower grades of animals. In both kingdoms there may be separation of the offshoots, or indifference in this respect, or continued and organic union with the parent stock; and this either with essential independence of the offshoots, or with a subordination of these to a common whole, or finally with such subordination and amalgamation, along with specialization of function, that the same parts, which in other cases can be regarded only as progeny, in these become only members of an individual.

This leads to the question of individuality—a subject quite too large and too recondite for present discussion. The conclusion of the whole matter, however, is that individuality—that very ground of *being* as distinguished from *thing*—is not attained in Nature at one leap. If anywhere truly exemplified in plants, it is only in the lowest and simplest, where the being is a structural unit, a single cell, memberless and organless, though organic—the same thing as those cells of which all the more complex plants are built up, and with which every plant and (structurally) every animal began its development. In the ascending gradation of the vegetable kingdom, individuality is, so to say, striven after, but never attained; in the lower animals it is striven after with greater though incomplete success; it is realized only in animals of so high a rank that vegetative multiplication or offshoots are out of the question, where all parts are strictly

members and nothing else, and all subordinated to a common nervous centre—fully realized, perhaps, only in a conscious person.

So also the broad distinction between reproduction by seeds or ova and propagation by buds, though perfect in some of the lowest forms of life, becomes evanescent in others; and even the most absolute law we know in the physiology of genuine reproduction—that of sexual cooperation—has its exceptions in both kingdoms in parthenogenesis, to which in the animal kingdom a most curious series of gradations leads. In plants, likewise, a long and most finely graduated series of transitions leads from bisexual to unisexual blossoms; and so in various other respects. Everywhere we may perceive that Nature secures her ends, and makes her distinctions on the whole manifest and real, but everywhere without abrupt breaks. We need not wonder, therefore, that gradations between species and varieties should occur—the more so since genera, tribes, and other groups into which the naturalist collocates species are far from being always absolutely limited in Nature, though they are necessarily represented to be so in systems. From the necessity of the case, the classifications of the naturalist abruptly define where Nature more or less blends. Our systems are nothing, if not definite. They are intended to express differences, and perhaps some of the coarser gradations. But this evinces, not their perfection, but their imperfection. Even the best of them are to the system of Nature what consecutive patches of the seven colours are to the rainbow.

Now the principle of gradation throughout organic Nature may, of course, be interpreted upon other assumptions than those of Darwin's hypothesis—certainly upon quite other than those of materialistic philosophy, with which we ourselves have no sympathy. Still we conceive it not only possible, but probable, that this gradation, as it has its natural ground, may yet have its scientific explanation. In any case there is no need to deny that the general facts correspond well with a hypothesis like Darwin's, which is built upon fine gradations.

We have contemplated quite long enough the general presumptions in favour of a hypothesis of the derivation of species. We cannot forget, however, while for the moment we overlook, the formidable difficulties which all hypotheses of this class have to encounter, and the serious complications which they seem to involve. We feel, moreover, that Darwin's particular hypothesis is exposed to some special objections. It requires no small strength of nerve steadily to conceive not only of the diversification, but of the formation of the organs of an animal through cumulative variation and natural selection. Think of such an organ as the eye—that most perfect of optical instruments—as so produced in the lower animals and perfected in the higher! A friend of ours, who accepts the new doctrine, confesses that for a long while a cold chill came over him whenever he thought of the eye. He has at length got over that stage of the complaint, and is now in the fever of belief, perchance to be succeeded by the sweating stage, during which sundry peccant humours may be eliminated from the system.

For ourselves, we dread the chill, and have some misgivings about the consequences of the reaction. We find ourselves in the "singular position" acknowledged by Pictet,—that is, confronted with a theory which, although it can really explain much, seems inadequate to the heavy task it so boldly assumes, but which nevertheless appears better fitted than any other that has been broached to explain (if it be possible to explain) somewhat of the manner in which organized beings may have arisen and succeeded each other. In this dilemma, we might take advantage of Mr. Darwin's candid admission that he by no means expects to convince old and experienced people, whose minds are stocked with a multitude of facts all viewed during a long course of years from the old point of view. This is nearly our case.

The Cutting Ant of Texas (Œcodoma Mexicana, *Sm.*).
By S. B. Buckley.

These Ants have homes under ground. In order to kill the ants, great excavations were made. Their extent almost exceeds belief, but they were seen by hundreds of the citizens. The underground rooms are rounded or oblong cavities connected by cylindrical passages from 1 to 3 or 4 inches in diameter. Some chambers are 6 inches wide by nearly as many in height, others 12 inches. In a clayey soil these chambers are walled by a thin dirty-brown wax-like secretion. The lowest chambers are generally 10 or 12 feet deep, while the upper cells are rarely nearer the surface than 18 inches. I extended a tape line down to the bottom of one, and found it 17 feet deep; at one of their largest dens, a room was found 16 feet beneath the surface, and several others were at near the same depth. At that place the ground is dug out from 12 to 16 feet deep, extending over an area having an average diameter of 25 feet, all of which was filled with ant-cells. Several large avenues (4–5 in. diam.) entered the bottom of this large den. On striking an avenue, some ants were seen to enter it followed by others, loaded with barley, all coming from that underground passage. Where they got the barley was the question, which was finally solved by going to a stable more than 300 feet distant, from which ants were seen to descend, each with his barley-grain, and enter a hole in the ground near the base of the stable, which was the only place in the vicinity where there was any barley. Another avenue on the other side is said to come out at the bank of a stream, between 200 and 300 feet distant, where are some elm-trees, from which the ants obtained bits of leaves, and carried them through the said avenue into the base of the den. That they have extensive underground passages there is not the least doubt. A gentleman recently told me of an instance where they dug under or tunneled a stream to get into a garden. There was a large ant-den on the other side of the stream, and for a long time the garden was safe from their depredations; but finally the Cutting Ants were seen there, carrying bits of leaves into a small hole in the ground. There was no ant-den in the vicinity, except the one across the creek; and as there were no dirt-heaps on the surface of the ground in the garden, as there always